

cruise down the Sea of Aral, and up the Amú, and, as we have said, a journey across the dreary desert of Kizzel Koom. Major Wood conveys, we think, a clearer and more vivid idea of the region indicated, its aspects, and its inhabitants, their characteristics and habits, than any other author we know. The maps which accompany the volume are a great assistance. We may note that they give the present level of the Caspian as 85 feet below that of the ocean, Lake Aral being 158 feet above sea-level. This, we presume, may be taken as authoritative for the present, and it ought to be noted, as the statements on the point in various authorities differ in a most remarkable way.

Major Wood naturally speaks of the conduct of Russia in Asia with warm approval, and indicates several beneficial results which have followed her recent conquests. He believes that of all European powers she, partly from the simplicity of her Government, and partly on account of her ethnic affinities, is best suited to wean the wandering hordes of Central Asia to a settled and civilised life. We strongly recommend Major Wood's work as one of substantial value and great interest. But why has a work of such importance and so full of details, been allowed to go forth without an index. We hope this omission will be remedied at the first opportunity.

OUR BOOK SHELF

La Théorie des Plantes Carnivores et Irritables. Par Edouard Morren. (Bruxelles: F. Hayez, 1876.)

In this pamphlet, a report of an address given at the annual public meeting of the scientific section of the Royal Academy of Belgium, on Dec. 16, 1875, Prof. Morren gives an admirable *résumé* of the present state of our knowledge on these two branches of vegetable physiology. As regards the now well-known phenomena of carnivorous plants, he gives the most essential points of the observations of Darwin, Hooker, Lawson Tait, Reess and Will, the author himself, and others: and, in contrast to his relative, M. Charles Morren, he gives his full adhesion to the view that nitrogenous substances are actually digested by the leaves of *Drosera*, *Pinguicula*, and *Nepenthes*. He points out, indeed, that the theory is not a new one, having been promulgated by Burnett in 1829, as respects *Sarracenia*; and by Curtis in 1834, and Canby in 1868, as to *Dionæa*; and also, he might have added, by Dr. Lindley, in his "Ladies' Botany," published in 1834. In his introductory remarks Prof. Morren insists on the identity of the process of nutrition in the animal and vegetable kingdoms. The second portion of the discourse is devoted to the elucidation of the phenomena of "Motility" as exhibited in the irritability of the leaves of *Mimosa*, the stamens of *Berberis*, and other organs which exhibit similar peculiarities; the aggregation of protoplasm as seen in the "tentacles" of *Drosera*; the apparently spontaneous movements of zoospores, climbing plants, &c. Anyone desiring to obtain a general idea of what is at present known on these interesting subjects could not do better than consult Prof. Morren's lecture. It is pleasant to find a tribute to "la science Anglaise" in connection with vegetable physiology.

A. W. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Supposed New Laurentian Fossil

WHEN a man finds that he has made a mistake, the best thing he can do is frankly to acknowledge and explicitly to correct it.

I lose no time, therefore, in making known to the readers of NATURE that the notice of a New Laurentian Fossil which I published in its columns three weeks since, was written under a complete misapprehension of the real nature of the body. So far from being calcareous, as I had been led to believe by the information I had received from the geologist who found the specimen, it proves to consist of alternating layers of felspar and quartz—the former simulating an organic structure like that of *Stromatopora*, and the latter occupying what had been supposed to be the cavities of that structure—together constituting what is known to petrologists as "graphic granite."

The conclusions I had drawn from a cursory examination of the sections first sent me by Mr. Thomson, instead of being confirmed by a more minute study of thinner sections, proved to be altogether untenable; what I had supposed to be piles of flattened chamberlets in the thickness of each lamella, turning out to be mere fissures in the felspar, arranged with extraordinary regularity; and what had seemed to be a vertical tubular structure, proving to be mere striation.

The examination of numerous sections of this body, and a comparison of them with sections of the "graphic granite" found in its neighbourhood, has now satisfied me that the former presents no other indication of organic origin, than is afforded by the *Stromatopora*-like disposition of its alternating lamellæ; and that this is so nearly approached in the latter, as to show that the agencies which produced the "graphic granite" were competent to have produced the supposed Harris fossil.

Whether these agencies were entirely inorganic, or whether the "graphic granite" itself may not be a metamorphic form of an ancient organic structure (metamorphoses nearly as strange having undoubtedly happened), is a question which is not at present to be decided by anyone's *ipse dixit*. When a petrologist shall have succeeded in making a graphic granite, he will be entitled to speak with assurance of its purely mineral nature.

It will doubtless be triumphantly urged by those who maintain *Eozoon* to be a "pseudomorph," that as I have had to confess myself completely mistaken in regard to the Harris specimen, I am just as likely to have been wrong in regard to the Canadian opicalcite. To this I have simply to reply that my mistake in the present case has arisen entirely from undue haste, and has been corrected by my own more careful study; which has satisfied me of the *entire absence*, in the Harris specimen, of those Foraminiferal characters which seem to me unmistakably recognisable in the Canadian *Eozoon*.

In the memorable discussion at which I was present in Paris, on the flint implements found associated with the Abbeville jaw, it was the *entire absence*, on the surface of those worked flints, of the staining, the dendrites, the patina, and the wearing of the edges, characteristic of the genuine implements, which satisfied the English experts of the factitious character of the former. But, so far from anyone being led by this discussion to call in question the fashioning of the genuine implements by men coeval with the river-gravels of the Somme, it only brought out more fully the strength of that case, by showing what complete reliance might be placed upon the characters of antiquity which they presented. And so, in the present instance, the striking contrast in the microscopic appearances presented by two bodies bearing a close resemblance in general structure, seems to me only to bring out the organic characters of the one more decidedly, by comparison with the purely mineral characters of the other.

WILLIAM B. CARPENTER

Theory of Electrical Induction

I WAS hoping someone of eminence would tell us what he thought of the arguments of Prof. Volpicelli, or whether no clearer view of induction had been arrived at. Prof. Clerk Maxwell's letter of last week brings back the subject to its natural point of view to one whose ideas are based upon potential, but at the same time it leaves some points doubtful which have a particular bearing on the whole theory. Might I therefore be allowed to ask information from him, by explaining the ideas which have been impressed upon me about this, by reading his book "Electricity and Magnetism," though they are removed *toto calo* from the ideas expressed by the phraseology of Prof. Volpicelli, and that of the usual text-books.

We know nothing of electricity except as a force. We may speak of it as a fluid, and use a corresponding terminology, but it is always measured as force. A conductor is a body in which

these forces immediately equilibrate themselves at the expense of calling into play other forces of the same or of opposite kind amongst the molecules of the dielectric. These forces give rise to the diminishing potentials as they are equilibrated over greater and greater surfaces. When another conductor is brought into the neighbourhood, since throughout it the electrical forces are in equilibrium amongst themselves, the various molecular forces are as before manifested only at the surface, and they are necessarily negative where the conductor protrudes into regions of higher positive potential than its own mean, and positive where it lies in the regions of lower positive potential. But not only this, the molecular forces which keep the electrical forces in the dielectric in equilibrium cannot thus simply be pushed, as it were, backwards and forwards, but must fall into equilibrium in their own way—in other words there is a redistribution of electricity both on the inductor and inducer, which can only be determined by properly drawing the equipotential surfaces corresponding to the new arrangement (if possible). The state of stress of the particles of the dielectric surrounding any small conductor is not affected by its total motion of translation, except that as it is moved from the other conductors it is redistributed on the surface.

If now we draw a series of equipotential surfaces, that particular one which corresponds to the potential of the conductor will divide it, as Prof. Clerk Maxwell says, into two parts, on one of which is negative electricity, and on the other positive, in other words the state of stress of the particles outside the conductor is of one kind on one side, and of the opposite kind on the other. Now comes my first question. If this is the case how can it be said that there is either more positive electricity on the inducer nearest the inductor as Prof. Clerk Maxwell says, or less as Prof. Volpicelli says, than at the other end, when in fact there is none, but the force is negative? No doubt we can take for mathematical purposes a negative quantity as the sum of two others, one positive and the other negative and greater, but can the existence of the positive quantity be called a "fact" in consequence?

There is a way, however, in which we might be inclined to say that the positive electricity is least nearest the positive inductor, but this looked at in the same way as before, raises a second question. If we make a small conductor touch any part of the induced conductor, and then try it in the usual way, we might say that the spot on which we touched it when the small conductor was most electrified had the greatest amount of electricity upon it, and might determine its kind. But before doing this we ought to ask what will be the effect of bringing the new conductor into the neighbourhood, and this depends on its shape and size. The equipotential surfaces will all be altered, and the alteration may be such that the one belonging to the first induced conductor may leave the new one entirely on the positive or entirely on the negative side, or may divide it into two like the first induced conductor. In connecting with the earth we make the new conductor so large that the old one is all on the negative side; and the fact that by breaking contact we can keep the old conductor charged with negative electricity shows that we may take any smaller part from the wholly negative side and it will also show the same electricity, as in inductive machines. If the new conductor be so shaped or so large that it cuts through the neutral equipotential surface, on removing it only the balance of the forces called into play will be left to be equilibrated by the molecular forces, and that balance may be positive though the contact was on the negative side of the former neutral surface. In this way only could a finite conductor take positive electricity from the negative side, but in this case it is due to induction on the new conductor as temporarily forming part of the old, and not to the original induction on the first conductor. What experimental proof, then, is there, or can there be, if these principles are true, that there is *any* positive electricity nearest the positive inductor before the distribution is disturbed by too long or large a conductor being brought into the field? and how, therefore, is Melloni's theory true?

Also, might not a point if properly placed on the negative side, cut through the neutral equipotential surface and so discharge positive electricity?

I should be glad to know, from a good authority, that we may thus explain these phenomena by a reference to force alone and not to hypothetical fluids, and without meddling with such useful, perhaps, but unmechanical ideas as "bound" and "free."

Dynamometers and Units of Force

IN NATURE (vol. xiv., p. 29) Prof. Barrett says "it would be interesting to know on what grounds Prof. Hennessy bases his emphatic and reiterated assertion." The assertion referred to is contained in my former communication (NATURE, vol. xiii., p. 466). The grounds on which it is based are as follows:—In order to accurately measure units of force according to the C. G. S. system, spring balances which could be depended upon to the $\frac{1}{1000}$ of a gramme or $\frac{1}{100}$ of a grain nearly would be required. In mechanics the forces to be compared and measured usually amount to several kilogrammes, and powerful spring dynamometers are most suitable for their estimation. Dynamometers such as those alluded to as being sent for exhibition from the College of Science to South Kensington are of this kind. By experiment I have found them unfit for the estimation of small units of force. I should be much interested in seeing Prof. Barrett or Dr. Ball measuring a C. G. S. unit or $\frac{1}{1000}$ of a gramme by the aid of one of these dynamometers. It should be remembered that in this discussion I all through refer to these dynamometers and others of a similar kind employed in mechanics. I was already aware of the belief expressed by Sir William Thomson and Prof. Tait, that spring balances, "if carefully constructed," would rival or even surpass the ordinary balance. While thus referring to the possible perfection of the spring balance with the qualifying particle "if," they justly remark that the pendulum is the most delicate of all instruments for the measurement of force. A pendulum will probably always furnish the best means for measuring force in absolute measure, whether by large or small units; and I entertain strong doubts as to whether the spring balance can ever supersede the beam balance for accurate determinations of weight. In no department of experimental inquiry are such minute quantities weighed, and nowhere is greater accuracy in determining differences of weight required than in chemical analysis, and chemists almost universally employ the beam balance in preference to the spring balance in their most delicate analytical researches.

In my former communication I mentioned that the dynamometers alluded to could not be depended on within the tenth of a kilogramme. In saying this I have spoken of them in the most favourable terms, for the larger one can scarcely be depended upon within the fifth of a kilogramme.

Prof. Barrett quotes a statement as "occurring in Prof. Hennessy's own syllabus," which implies that I had adopted and used the C. G. S. system. The words quoted belong to a syllabus written by Dr. Ball for the session 1874-75. I entered on my duties after the commencement of that session, and my name was attached to new editions of the syllabus instead of the name of its author, while the part of the syllabus relating to mechanics remained untouched. I had been always under the impression that Prof. Barrett was perfectly aware that I was not the author of this syllabus, and although technically it might be regarded as the syllabus of applied mathematics in the College until a new one could be prepared and published with the sanction of the Science and Art Department, it seems scarcely correct in a scientific discussion to quote it as expressive of the views of a person who was well known not to be its author.

Prof. Barrett, in his first letter, laid much stress on the introduction of spring dynamometers into Dr. Ball's courses on mechanics for the estimation of force in absolute measure; as if such an employment of these instruments was entirely new. It is but just to observe that dynamometers of the same kind, and graduated in the same way, have been long since employed in other courses of mechanics, and such instruments are figured and described in some of the most common elementary books used in the colleges of Europe. With reference to the dynamical units which I prefer to employ in my courses of mechanics, Prof. Barrett uses the phrase, "a mixed system of kilogram-meters and foot-pounds." I never mix the two kinds of units. I keep them perfectly distinct. I employ both, because in the practical applications of mechanics, students may be called upon to apply one or the other. As far as I have been able to ascertain, these are the units in most general use among engineers throughout the world; and I should as soon expect mechanicians to adopt the C. G. S. system as to hear that bankers adopted our smallest coin as the unit of account instead of the sovereign, and to see the prices of stocks in the money market no longer quoted in pounds but in farthings.

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